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Interactive comment on "Predicting discharge capacity of vegetated compound channels: uncertainty and identifiability of 1D process-based models" *by* Adam Kiczko et al.

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1 Major comments

Referee #1's major comments:

The paper addresses an important and highly relevant topic and is of great importance for the professional community to improve the capabilities to accurately model the complex flow in vegetated compound channels. However, I identified various issues that should be addressed and clarified by the authors. In general, the paper has been written with great care, but there are many passages that should be improved stylistically

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Discussion paper

and where the language should be improved (see my detailed comments below; I recommend that the language is cross-checked by a native speaker). I also found that the terminology should be better introduced and defined to help the reader to better understand the complex content of the manuscript. For example, it became not really clear to me what is meant by, for example, identification data points, verification data points, computation points, observation points etc. Overall, the manuscript focuses on a statistical analysis of different approaches regarding uncertainties associated with input parameters. This is highly relevant, but the manuscript is mostly written from a statistical (probabilistic) point of view. However, in practice the tested approaches are typically used by hydraulic or environmental engineers, and it would be nice to outline the chosen approach more generally at the beginning of the manuscript, so that the significance of the results becomes clearer for the target-audience. In this context, I am not an expert in statistics and this fact triggered many questions (see below). In my opinion, this manuscript could make a real impact if it would be written in a way that practitioners will better understand what has been done. Also, more specific statements related to the parameter variation would be helpful. I acknowledge that this is partly addressed in the discussion, but it becomes not really obvious from the preceding sections. To summarize, *this is an interesting manuscript.* However, the presentation of the material should be improved. As I have many specific comments, I am afraid that I have no other choice than to recommend returning the manuscript to the authors for major revisions. I hope the authors will find my comments useful.

Response: We are pleased to hear that the reviewer acknowledges the relevance and importance of our work, and are very grateful to the reviewer for his comments. Especially, we would like to make this article interesting for practitioners, and we acknowledge the reviewer's many remarks on the clarity of this study. We agree that the study would benefit from extending the focus from statistics to practical implications/significance, and the manuscript will be revised accordingly, paying attention to introducing the concepts related to the uncertainty more clearly already in the Introduction. If the manuscript is considered for revision, we would like to address comments

by providing better explanations of the study scope, definition of the terminology used, and a clearer presentation of the applied methodology of uncertainty analysis. We will have the language checked.

We also agree with the reviewer's suggestion, that more specific statements should be given in the case of a priori parameter variations. This issue was also mentioned by other reviewers, and we are sure, that we should provide a detail explanation of our approach in that matter. First, we would like to introduce term of uninformative parameter bands for the a priori distribution. We think it is the only way to compare different methods, as we assume that the modeler has no prior knowledge on channel resistance. Moreover, that parameter ranges should be large enough to ensure that the highest probability region of the probabilistic solution is not affected by them. We would like to include in the methodology section a subsection, where these issues would be explained.

2 Specific comments

We provided answers along with reviewers specific comments:

1. Title: I am not sure that I understand what is meant by "identifiability" – I guess a "statistical" meaning is meant, but that becomes not clear.

   Response: With "identifiability" we refer to the possibility of finding a distribution of model parameters that explains its uncertainty and follows physical constraints set on the parameter values, assuming that the modeler's knowledge is restricted only to water level and discharge measurements. Brief explanations can be given immediately in the abstract. We will consider changing the title of the manuscript to something more generally understandable.

2. P2, L22: What is meant by stability?

Response: Morphological stability of the channel system and this way the sentence should be clarified.

3. P2, L25: I find this a bit confusing, as there is some redundancy with the sentence before. This could be formulated better.

   Response: We will revise lines 22-25 as: “Such nature-based solution (NBS) allow combining the technical needs, e.g. [FB02?]ow conveyance and stability, and the environmental requirements, e.g. improved water quality and biodiversity (Rowinski et al., 2018), but require reliable predictions on the discharge capacity. Predictions using the conventionally applied methods (e.g. Posey, 1967) can . . . ”.

4. P2, L27: What exactly is meant by “regions”? I assume the different channel parts are meant (but one could also think about different geographical regions).

   Response: The reviewer is right, the term “parts of channel with highly different flow resistance” will be more appropriate and will be used in the revised manuscript.

5. P2, L33: Is this approach really simple? I would delete the latter word.

   Response: Comparing to Pasche and similar methods DCM is very simple, but we understand that this appears before introducing these methods. The word should be removed.

6. P2, L36: Please check language.

   “Despite the development of more advanced methods, providing often much more detailed and physically based description of channel flow resistance, DCM is till this day found in the majority of practical models for flood hazard assessments, design of hydraulic structures or water management.”

   Response: We will revise the sentences so that the text will flow better: “DCM is presently the basis for the majority of practical models for flood hazard assessments, design of hydraulic structures and water management.”

7. P2, L41: Which relationship is meant here?

   “It should be noted, that this relationship can be amplified with inadequacy of a flow model, as mentioned by Yen (1999)”

   Response: The sentence should be rewritten: “It should be noted, that such dependence on flow can be amplified with inadequacy of a flow model (Yen 1999).”

8. P2, L42 & P2. L43: Check language
   “A number of studies were devoted developing a more process-based description of channel flows (Yen, 2002).

   Response: The 2 sentences will be revised to: “A number of studies were devoted to developing a process-based description of either the channel flow processes or the interactions with elements obstructing the flow, such as vegetation (Yen, 2002).”

9. P2, L44: Why “should”?

   R**esponse:** Instead of “should”, we will write: “... the most sophisticated model of the channel capacity can be attributed to Shiono and Knight (1991),”

10. P2, L47: The approach was published in 1991, and the only reference for its successful use is from 2020? I doubt that it took 30 years that it was successfully used...

    Response: The reviewer is right that we should have included here other references, as well, e.g.:

    - Abril, J.B. i Knight, D.W. (2004). Stage-discharge prediction for rivers in flood applying a depth-averaged model. Journal of Hydraulic Research, 42 (6), 616-629.;

- Babaeyan-Koopaei, K., Ervine, D.A., Carling, P.A. i Cao, Z. (2002). Velocity and Turbulence Measurements for Two Overbank Flow Events in River Severn. Journal of Hydraulic Engineering, ASCE, 128 (10), 891-900.
- Kordi, H., Amini, R., Zahiri, A. i Kordi, E. (2015). Improved Shiono and Knight method for overflow modeling. Journal of Hydrologic Engineering, 20 (12), 04015041.
- Sharifi, S., Sterling, M. i Knight, D.W. (2011). Can the application of a multi-objective evolutionary algorithm improve conveyance estimation? Water and Environment Journal, 25 (2), 230-240.
- Shiono, K. i Rameshwaran, P. (2015). Mathematical modelling of bed shear stress and depth averaged velocity for emergent vegetation on floodplain in compound channel. In: E-proceedings of the 36th IAHR World Congress, 28.
- Tang, X. i Knight, D.W. (2008a). A general model of lateral depth-averaged velocity distributions for open channel [FB02?]ows. Advances in Water Resources, 31, 846–857.
- Tang, X. i Knight, D.W. (2008b). Lateral depth-averaged velocity distribution and bed shear in rectangular compound channels. Journal of Hydraulic Engineering, 134 (9), 1337–1342.
- Tang, X., Sterling, M., i Knight, D. W. (2010). A general analytical model for lateral velocity distributions in vegetated channels. River Flow 2010, 469-475.
- Zhang, J., Zhong, Y. i Huai, W. (2018). Transverse distribution of streamwise velocity in open-channel flow with artificial emergent vegetation. Ecological Engineering, 110, 78-86. doi: 10.1016/j.ecoleng.2017.10.010

11. P2, L48: Please check language

Response: The term “physically-based” should be changed to “physics-based”.

12. P2, L49: Why referring specifically to Pasche, and not to Pasche & Rouvé?

    Response: The reason is that, the methodology was developed by Pasche and this is the only English article presenting its basis. The reviewer is however right and to avoid confusions we shall include also references to original Pasche works in German.

13. P2, L53: Please check language

    Response: Sentence should sound: "**A** simplified version of the method was proposed by Mertens (1989)."

14. P2, L55: Check how reference is included into the text.

    Response: Thank you for pointing this out.

15. P3, L57: I doubt that all of the cited approaches have been developed to parametrize the two-layer approach. Some of them deal more with the parameterization of vegetation properties.

    Response: A straight-forward two-layer flow description was proposed by Luhar and Nepf (2013) while Västilä and Järvelä (2018) generalized the approach, incorporating a parameterization of flexible foliated vegetation (Västilä and Järvelä 2014; Jalonen and Järvelä 2015).

16. P3, L58: Please check the sentence (". . . for in. . ..").

    Response: "for" should be removed.

17. P3, L60: Please improve "Methods like Pasche and Rouvé (1985). . ." Pasche and Rouvé are the names of the authors who developed the method (a similar comment can be given regarding the reference to Västilä and Järvelä in the same line).

    Response: We will revise the sentence (and similar sentences) to: "Methods such as those developed by Pasche and Rouvé (1985). . ."

18. P3, L62: Is this really true? Remote sensing methods have significantly developed, and dependent on regions and countries, such information may be available. . .

    Response: There are studies where remote sensing techniques have been used to estimate vegetation properties for hydraulic modeling. However, such studies present rather the state-of-the-art in research, while in many practical assignments the vegetation data is mostly insufficient for these approaches. We understand, that this point is confusing, and we will specify that this statement applies to the practice and will provide a comment on remote sensing techniques.

19. P3, L65: In my opinion, this is not an argument for simpler methods. Such a non-physical based black-box will not help to better understand the problems at hand and requires manual and arbitrary calibration.

    Response: We will clarify our message and revise the preceding and following sentences to: "With these practical limitations, the use of a roughness coef[FB01?]cient lumping all effects, such as the Manning coef[FB01?]cient, can provide a reasonable prediction (i.e. Marcinkowski et al., 2018, 2019). However, the common practical approach of adjusting the roughness coef[FB01?]cients to fit the model to observations may lead to the coefficients to be used beyond their physical interpretation, as discussed by Yen (1999) in the response to Khatibi et al. (1997)."

20. P3, L67: Why disused? What is meant by this?

    Response: We apologize for the spelling mistake. "Disused" should read "discussed", i.e.**,** we refer here to the exchange of discussion papers between Yen and Khatibi et al. We found it very relevant to presented study.

21. P3, L68: Fr typically defines the Froude number – but I guess here the word "For" is meant? I stop here giving particular comments on the language, as I already have provided many such comments showing the need to improve the paper.

Response: Apologies for misspelling, should be “For”. We will carefully check the language in the revised manuscript.

22. P3, L69: I do not understand this statement – “when bathymetric data do not account for the true complexity of the river geometry” – what is meant by “true complexity of the river geometry”?

    Response: We meant here the situation where assumption of the linear evolution of geometric features between two cross-section is not maintained. So i.e. when in reality between two cross-sections there are some irregularities in channel geometry, which are not shown by the the cross-sectional geometry data. We will address reviewer remark by revising our sentence to: “when cross-sectional geometry data do not account for the true complexity and irregularity of the river geometry between the cross-sections”.

23. P3, L70: This example is not really related to the topic of the paper. . .

    Response: We agree that it is not related to the vegetative flow resistance, but in our opinion this perfectly links with the problem of using formulas with parameters beyond their physical meaning.

24. P3, L72-75: This is difficult to understand – please improve.

    Response: We will clarify this sentence.

25. P3, L77: Which answer? In other words, what is the question?

    Response: We agree, the sentence should be rephrased as follows: “This leads to an old dilemma, where a simple model with limited number of parameters is compared with a complex one with more parameters (Kuczera and Mroczkowski, 1998), which can be addressed through the models’ predictive uncertainty.”

26. P3, L81: “. . .comparing to the Manning. . ..” – this part of the sentence remains unclear to me.

Response: It can be added for the clarity, “comparing to the Manning”.

27. P3, L86: I do not understand what is meant by “parameterized in a sense of their distributions“

    Response: The sentence should be rephrased. The chosen sources of uncertainty were described using probabilistic distributions, reflecting modelers’ expectations on possible variability of each input.

28. P4, L93:Improve stylistically (. . .Järvelä Järvelä. . .)

    Response: We apologize for the misspelling.

29. P4, L95: Which study? This one?

    Response: Should be “In this study”.

30. P4, L99: “The overall goal of this paper is to compare the uncertainty, parameter identifiability and physical interpretation of the parameters of discharge capacity methods characterized with different levels of parameterization”. This sentence is very difficult to understand.

    Response: The sentence can be rewritten as follows: “The overall goal of this paper is to compare the uncertainty, model identifiability and physical interpretation of the parameters of chosen discharge capacity methods. The challenge arises from different number of parameters in each model.”.

31. P5, Figure 1: Please improve the caption and the description of the figure; I find it difficult to understand (note also that not all parameters have been defined)

    Response: The caption will be improved.

32. P5, L120 – 125: Please improve – this could be explained more clearly in my opinion.

Response: Following also other reviewers remarks on the clarity, we agree that the section 2.1 should provide much better explanations of our approach.

33. P9, L242: ". . .are plant species. . ."? I am not sure that I understand what is meant.

    Response: Should be rephrased "are factors specific for plant species or plant type"

34. P9, L247: This depends on the level of submergence – otherwise 20% of the discharge may be neglected. . .

    Response: The reviewer is correct that with the simplified two-layer model (STLM), up to 20% of the discharge is neglected, depending on the density and cross-sectional blockage of vegetation. This share of discharge results from back-calculating from the original simplified approach proposed by Luhar & Nepf (2013). We will add this information to the text.

35. P9, L251: This depends - for the typically used rigid cylinder analogy, Bx will basically be constant.

    Response: We would like to remind that Bx represents the bulk-level cross-sectional scale distribution of vegetation. Thus, Bx of any type of vegetation will depend on the changes in the wetted cross-sectional area resulting from changes in water level. The effect will be particularly large when the water level rises above the height of floodplain vegetation, in which case the Bx typically starts to decrease.

    Note, that in general case, Bx value is also affected by the cross-section geometry.

36. P9, L253: What exactly is l_l and l_r? In this context, a sketch would be helpful.

    Response: If the manuscript is considered for the revision, we will include here a sketch illustrating the way how these parameters are applied.

37. P10, L264: The model of Luhar anf Nepf was already mentioned before - it may be a good idea to restructure the manuscript and to present this approach earlier? Also, this is not the “original formula” (which should be stated more clearly), as the hydraulic radius is used while Luhar and Nepf used the water depth.

    Response: We agree this is already modified model, however the modified Luhar-Nepf model was introduced with eq. 6-7.

38. P10, L269: Check writing style (...“formula 9...”, “...three parameter one...”) etc).

    Response: Thank you for pointing this out, we aim to be more specific. Lines 269-271 should read: “ Eq. (9) has a convenient form to be easily applied in practical cases, where usually the Manning equation is used. In the present study, this approach is called the Practical Two-Layer Model (PTLM) as it requires less parameters influenced by vegetation.”

39. P10, L280: Remove the full stop after “experiments”

    Response: Thank you.

40. P10, L290: Strictly speaking, uniform conditions are impossible to achieve by this setup - I would prefer if the terminology “quasi uniform” is used. The flume slope (or the slope range) should also be given - I could not find this information in Koziol (2010) as this paper seems to be in Polish language.

    Response: Reviewer is right, we will provide additional comment on the flow uniformity and references, where this issue for the WULS-SGGW flume was discussed. Also the information on the channel slope is missing and should be provided. For the the WULS-SGGW flume the slope was: s = 0.0005.

41. P10, L291: How were water levels recorded and what was the spacing between the measurements?

    Response: We used a pressure gauge, that allowed us to measure differences in depths between downstream and upstream sections of the flume at the distance

of 4.8 and 12 m from the inflow to the channel (the length of the channel was 16 m). The setup of the experiments in the WULS-SGGW flume is given in the recent articles of Kubrak et al. 2019 and less recent of Kozioł (2013). References and explanations should be included in the article text.

- Kubrak, E., Kubrak, J., Kuśmierczuk, K., Kozioł, A., Kiczko, A., & Rowiński, P. M. (2019). Influence of stream interactions on the carrying capacity of two-stage channels. Journal of Hydraulic Engineering, 145(4), 06019003.
- Kubrak, E., Kubrak, J., Kozioł, A., Kiczko, A., & Krukowski, M. (2019). Apparent Friction Coefficient Used for Flow Calculation in Straight Compound Channels. Water, 11(4), 745.
- Kozioł, A. P. (2013). Three-dimensional turbulence intensity in a compound channel. Journal of Hydraulic Engineering, 139(8), 852-864.

42. P11, L300: What kind of vegetation?

    Response: We will add the information that it was mainly grassed vegetation, consisting of different species with both stems and foliage.

43. P13, L320: What is constrain 5? Is equation 5 meant?

    Response: Yes, we meant constraint given with the eq. 5. The sentence should be rephrased.

44. P13, L328/330: Please use another notation for the number of observation points before, n was used to define the Manning coefficient, and this is confusing (see also my comment below; L336). In this context, what exactly is meant by observation point? It could also be the number of points Also, it should be mentioned that only floodplain flows were investigated (for convenience of the reader).

    Response: We agree with the reviewer, we will use another symbol for the number of observations. Also we will stress that calculations were performed only

for flows higher than bankfull condition. By the observations we meant the measured water level H and flow rate Q. Each "observation" consisted of a single pair of these values. We agree that this should be explained clearer.

45. P13, L334: Not necessarily in some (or many) cases there exist data for high flows that can be used for calibration.

    Response: We think this is a terminology issue and agree that the term "low" flow is inaccurate. In both cases we consider floodplain flows, but of low vs higher floodplain water depth, and we will rephrase the sentence accordingly.

46. P13, L336: I am partly confused here, n is the number of observation points which could also be the number of readings taken for the water depth measurement. Please be more specific.

    Response: As in previous response, we would like to clarify the terminology concerning the number of observations.

47. P13, L346: Please improve cross-references - e.g., here it should be Figure 4a. Figure 4d-f: Why are the lowest two points characterized by the same discharge?

    Response: These are two independent water level/discharge measurement and differences in water level value comes from measurement uncertainty.

48. P15, L349: I am not sure that I can follow - this could be explained better (what is exactly is meant by computation points).

    Response: As in previous response, we would like to clarify the terminology concerning the number of observations.

49. P16, Table 2 (also Table 1): Could you comment on the used parameter bands in the text (why were these bands chosen?)?

    Response: This issue was also raised also by another reviewer. We used "uninformative" ranges for parameters, although within physically interpretable bands.

Explanations given in the text are insufficient and we would like to add explanations in the methodology section, explaining our approach.

50. P16, L353: What exactly are identification data points?

    Response: It should be “observations used for identification”.

51. P16, L355: But the Pasche model results are not included in Fig. 5? This is confusing.

    Response: We apologize that the reference to the figure 6 is missing (and the the wrong case number was given)

52. P16, L356: (5a, 6a) - does this refer to the Figures? Please improve throughout the manuscript.

    Response: The reviewer is right that we should cross-check all references throughout the manuscript

53. P17, Figure 5: I am not sure that I understand what is meant by “Ratio”.

    Response: Probably the word “share” would be better.

54. P17, L365: This definition could have been given earlier.

    Response: We agree, the definition should be given and explained in the methodology section.

55. P17, L369: Here, equation is used, at other places formula is used and at some places numbers are just given. Please improve - this is confusing. Also, the used model could be specified more precisely (the same applies to “other unspecified models”).

    Response: We are sorry for causing such unnecessary confusion and will be consistent.

56. P17., L370: Now n is the ensemble count - this is confusing.

    Response: Should be number of observations.

57. P18, L374: I am not a statistical expert (although I have some knowledge regarding statistics), but this is a bit confusing....

    Response: We understand it is unclear. In other words, we meant, that with that model it was impossible to reproduce a rating curve that was able to explain much more than a single observation point.

58. P18, L378: Why is 1 an extreme value?

    Response: The sentence is unclear, it should be explained that, for that model only for some sets it was possible to find a solution that was able to explain several observation points.

59. P18, L380: This fact could be explained in some more detail when outlining the approach. I am getting a bit lost here...

    Response: Our idea to address the reviewer remarks on the clarity of presentation is to add a section in the methodology, supplemented with a schema, explaining our approach.

60. P18, L383: Vegetation characteristics of Ritobacken have not been defined; what is meant by a “flexible approach”?

    Response: The vegetation in the Ritobacken should be considered as flexible. The vegetation characteristics will be included in the description of the Ritobacken case study.

61. P19, L396: I have trouble understanding this - a more general outline of the procedure would be helpful (this should be provided earlier, not here in the presentation of the results).

P26, L416: I am not sure that I understand what is meant here. I stop giving more comments here on chapter 3 as I have problems to understand what exactly was done - the procedure could be outlined in some more detail

P31, L492-498: It would be good to explain all his earlier in some more detail.

Response: We thank the reviewer for helping us to understand that we need to add a detailed explanation of our approach in methodology section.

62. P31, L508: Isn't this rather obvious? By the way, what about errors in the measurements - how would they affect this analysis?

    Response: It is true, but with this statement, we would like to show that we were able to reproduce this effect using our approach. In the case of measurement uncertainty: we should mention in the methodology section, that we analyze the total uncertainty - of the model and also measurement. With our approach it is impossible to distinguish these two sources. The remark should be addressed with a detailed explanation of the adopted uncertainty analysis.

63. P33, L578: Numerical experiments are mentioned - but I doubt that detailed numerical simulations were carried out (no statements are given in the manuscript); this again shows the need to formulate statements more precisely.

    Response: The remark links to the clarity in presenting our approach. We hope, we would be able to explain it better with additional subsection of the methodology.

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